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~~Dear Dr. Luria,~~
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Just a few remarks shimmering from our conversation in Minneapolis. I suppose that you are quite right that there are some obscurities in the mechanism of recombination in K-12 that have to be cleared up. In time, I shall certainly be attending to it, but I still don't think that so-called kinetic experiments will contribute very much. However, I have repeated "timing" the occurrence of the recombinant prototrophs against wild type cells put on the same medium at the same time, and find that my earlier statement is quite incorrect. There is a difference amounting sometimes to more than 24 hours between the growth of wild type and the development of prototrophs. There is also certainly a residual background growth of the mutants inoculated, but ~~exactly~~ enough to allow the development of double-reverse-mutations. I hope I don't have to go into that again. Your calculation that inoculating, say, 10^7 cells per cc. will result in a mean distance between "cells" (i.e. the centers of microcolonies) of 25 u or more is quite correct ($100 \frac{2}{3}$ or 46 u). However, it must be obvious that if there is a random distribution of distances, the proportion of cell pairs with a distance of less than x will be given by $1 - e^{-a}$, where a is $x/46$ to the third power. For small values of a , this is ~~max~~ merely a , and, for example, .1% of the "cells" will be paired at a distance of 4.6 u. Considering the formation of microcolonies, this leaves ample room for the relatively rare occurrence of cell fusions, and I can see no contradiction on these grounds. To go ahead and prove that just this happens may be another story. It is not so simple as varying the concentrations of the "reactants", because a) the physiological condition of the cells and b) the size of the microcolonies depends on the inoculum size. Selection with phage may be a better system.

But what is strangest of all to my mind is the suggestion that has been offered that transforming factors are a more likely interpretation. If so, these substances will have to possess remarkable properties, even beyond those of the pneumococcus system; But I imagine that the basis of this suggestion is the quirk that some bacteriologists have for demanding that bacteria are a law unto themselves and that what holds (i.e. sexuality) for anything else cannot be true of a bacterium. And of course, they may be right.